CONSTRUCTIVE EMPIRICISM AND SCIENTIFIC PRACTICE. A CASE STUDY

Valeriano IRANZO*

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* Departamento de Lógica y Filosofía de la Ciencia, Universidad de Valencia, Avda. Blasco Ibáñez 30, 46010 Valencia. E-mail: Valeriano.Iranzo@uv.es

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ABSTRACT: According to van Fraassen, constructive empiricism (CE) makes better sense of scientific activity than scientific realism (SR). I discuss a recent episode in bio-medical research -investigations about *Helicobacter pylori* and its relation to peptic ulcer. CE's expedient to cope with it is a sort of belief substitution. I argue that replacing realist beliefs by empiricist surrogates (as-if beliefs) could accommodate scientists' expectations and behavior. Nonetheless, theoretical agnosticism could hardly motivate scientists to focus just on the observational consequences derived from the theory at issue. Contrary to van Fraassen, I conclude then that, concerning scientific practice, realist beliefs cannot be considered as a gratuitious surplus which should be rejected.

Keywords: constructive empiricism, scientific practice, scientific realism, van Fraassen, bio-medical sciences (philosophy of).

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0. Introduction

The soundness of a philosophical interpretation of science partly depends on its ability to make sense of scientific practice. In *The Scientific Image* van Fraassen affirms that constructive empiricism (CE) must be preferred to scientific realism (SR) because "it makes better sense of science, and of scientific activity, and does so without inflationary metaphysics" (van Fraassen 1980, p. 73). This paper aims to assess the plausibility of this assertion.

As a preliminary step, it may be worth summarizing the fundamental theses of CE regarding this issue (ibid., ch. 2, sec. 1):

(a) Science aims to give us theories which are *empirically adequate*. A theory is empirically adequate when it fits with all -past, present and future- observable phenomena. In other words, a theory is empirically adequate iff all its observational consequences are true.

(b) Believing a scientific theory is not the same as *accepting* it. Believing a theory means believing that it is true as a whole. By contrast, accepting a theory does not require more than believing that all its observational consequences are true. Certainly, accepting a theory usually requires believing that it has some other virtues besides empirical adequacy (explanatory power, fertility,...). Van Fraassen acknowledges the important role played by those virtues, but he considers they are only pragmatic virtues, that is, they are utterly irrelevant in respect of the theory's truth.

(c) Scientific realism is needless in order to account for theory acceptance and its related consequences in scientific practice.

Assertions (a) and (b) reveal that the observable/unobservable distinction is essential to formulate CE. Logical empiricists applied this controversial distinction to statements, but van Fraassen prefers speaking about *entities*. The examples provided in *The Scientific Image* suggest the equivalence between 'observable' and 'what could be observed with the naked eye'. Thus, although we cannot observe some planets without using a telescope, planets are, notwithstanding, observable things because we could see them without instruments if we were close enough. By contrast, viruses or electrons are paradigmatic examples of unobservable entities, because we could not see -or detect- them without microscopes.

The observable/unobservable distinction has been strongly criticized in recent decades, and van Fraassen's proposal has not been excepted.[1] His is, indeed, a very rough distinction taking into account the fundamental role it plays in CE. However, it is important to notice that for van Fraassen the distinction has only epistemic consequences. He does not assert that unobservable entities do not exist. Nor does he defend a kind of semantical indetermination for statements about such kind of entities -they are either true or false, just in the same way as statements about observable entities. Rather, his point is that accepting a theory requires neither believing it is true nor believing that the unobservable entities it postulates exist. Moreover, suspension of belief in unobservable entities -*theoretical agnosticism*- is

not only possible in scientific contexts, but desirable as well. In fact, realist working scientists are going beyond what "science itself involves or requires for its pursuit" (van Fraassen,1994, p. 182). Thus, as assertion (c) states, SR is worthless with respect to scientific practice.

I allude to the observable/unobservable distinction here only to the extent that it may be relevant to characterize van Fraassen's standpoint, since I want to focus on the prospects for theoretical agnosticism concerning scientific activity. In section 1, I shall make use of an example taken from recent medical research to illustrate the SR/CE debate. The plausibility of a constructive empiricist reading of this particular episode will be discussed in sections 2, 3 and 4. CE's expedient to cope with scientific practice is a sort of belief replacement. I shall argue, however, that replacing realist beliefs by empiricist surrogates (as-if beliefs) would not leave current scientific practice untouched. Granted that, it can hardly be claimed that CE makes better sense of scientific activity than SR. The moral is that realist beliefs cannot be considered as a gratuitous surplus which working scientists should reject. In the last section -section 5- I claim that, in fact, CE demands a different way of doing science.

1. Bacteria

In 1983 an australian pathologist called J.R. Warren reported that he had found a kind of "small curved and S-shaped bacilli" in gastric biopsy specimens. Bacteria could be observed using a silver staining method. They were rare when there was no inflammation, but they were often found in chronic gastritis, and its number still increased in active chronic gastritis. He could not identify the bacterium -he just noticed that it resembled *Campylobacter jejuni.* Warren's conclusion was that the microorganism should be recognized and their significance investigated, since it was "closely associated with an active form of gastritis, a cause of considerable morbidity (dyspeptic disease)" (Warren and Marshall 1983, p. 1273).

A report of B. Marshall -one of Warren's colleages at the Royal Perth Hospital- was attached to Warren's letter. Marshall noticed also that the presence of related microorganisms in the human stomach, as well as in other mammals' gut, have been reported for a long time under the label of "spirochaetes". Nevertheless, after having cultured Warren's bacterium, Marshall could give an account of its morphological structure sufficiently detailed as to recognize its specificity. He claimed that the bacterium was a new species and he suggested that it had been overlooked before because

Warren's staining method is not routine for mucosal biopsy specimens. The known microorganisms previously found had been considered non-pathogenic commensals. As for the new bacterium, Marshall conclusion was cautious:

> the pathogenicity of these bacteria remains unproven but (...) if these bacteria are truly associated with antral gastritis, as described by Warren, they may have a part to play in other poorly understood, gastritic diseases (i.e., peptic ulcer and gastric ulcer) (Warren and Marshall 1983, p. 1275).

In 1984 they accomplished a more detailed investigation. The antral mucosa of one hundred patients presenting for gastroscopy was examined. The bacterium was found in specimens from 58 patients and was present in almost all patients with active chronic gastritis, duodenal ulcer, or gastric ulcer. They suggested the hypothesis that the bacterium was "aetiologically related to chronic antral gastritis and, probably, to peptic ulceration also." (Warren and Marshall 1984). They noticed also that the new bacterium closely resembled campylobacters and, even though its flagellar morphology were not that of the genus *Campylobacter*, they proposed the name '*Campylobacter piloridis*'.

No doubt, Warren and Marshall's hypothesis had radical consequences.Traditionally, peptic ulcers have been associated with stress or inadequate diet. Until recently such factors were considered the main causes of the characteristic increase in acid production which eventually produces peptic ulcers. Acid-supressing drugs were the usual therapy. In fact, both "H_2 receptor blockers" and the newer "proton pump inhibitors" can heal ulcers in few weeks. However, duodenal ulcers often recur, so, in order to avoid recidives, doctors regularly prescribed a maintenance dose. Patients usually felt that they were suffering from a chronic disease which could awake as a result of a stressing situation, a seasonal change,... But, suddenly, a fairly common illness had to be reconceptualized as an infectious disease. The medical community at large poured scorn on the hypothesis, but some research teams found it interesting enough to invest time and resources to answer the many questions which inmediately arose: Is there the same strong correlation between infection and peptic disease in larger samples of patients? Does the bacterium cause the inflammation or it is the inflammation that makes easier for bacterium to grow in the gut? How can the bacteria survive in an acid environment? What is the role played by the

bacteria in the stomach's level of acid? Which is the way of contagion? Which is the best treatment?

In 1989 the Royal Perth Hospital team had enough evidence to justify the creation of a new genus, and they renamed it as *Helicobacter pylori.* Since then, at least eighteen other *Helicobacter* species have been found. Researchers discovered that the infection with *H. pylori* was widely spread in humans. It is present in almost half of the world population, with particularly high rates in developing countries. *H. pylori* strikes primarily in childhood via oral-oral or faecal-oral contact. Risk factors are overcrowding during chilhood and no fixed hot water supply. Certainly, only approximately 1/6 of infected people develops ulcers, so the final outcome does not depend only on having been infected. Further factors as bacterial genotype, host physiology, host genotype and diet play a role. The infection usually remains in the stomach for decades. In some cases it provokes a chronic gastritis and, then, a peptic ulcer. However, only 5% of those patients with duodenal ulcers are not infected.[2]

In addition to this, researchers found that, after eliminating the bacteria with antibiotics, ulcers did not recur. Perhaps this was the crucial fact for a medical community desirous of results. Initial skepticism gradually disappeared and more people and institutions became involved in research projects on *H. pylori.* In 1994 the US National Institutes for Health advised doctors to replace the conventional therapy by a new one: antibiotics. Ten years before this state of affairs would had been unthinkable, and it remained so until recently for a great number of physicians. Meanwhile, associations with other gastric diseases have being investigated. The bacterium is not only the main cause of chronic gastritis and duodenal ulcer. It has been found implicated in chronic atrophic gastritis (a precursor of gastric carcinomas), non-malignant gastric ulcers, and a kind of lymphoma (Goodwin *et al.* 1997). Also, there is some evidence to link the bacterium with persistent diarrhoea and increased susceptibility to other infectious diseases (Cover and Blaser 1996).

Since it is widely spread, *H. pylori* has a remarkable polimorphism. The most dangerous strains release a vacuolating toxin codified by the *cagA* gene. This gene was cloned in 1993. But during the nineties a more ambitious project -the task of sequencing *H. pylori*'s genoma- was completed by The Institute for Genome Research (TIGR). The sequence was published in *Nature* in 1997 (Tomb *et al.* 1997). Now we know which is the segment in the sequence containing the set of accessory factors for secretion of the toxins which damage the host gut. Trials on mice suggest the

possibility of elaborating a vaccine to eradicate the bacterium and new therapies using genetic manipulation may be developed in the next years.

The amazing success of Warren and Marshall's hypothesis had consequences in related fields. By the end of the eighties, it was discovered that people with coronary heart disease (CHD) has high levels of antibodies to *Chlamydia pneumoniae* (TWAR), suggesting that they have been repeatedly infected with the bacillus (Saikku *et al.* 1988). In fact, *C. pneumoniae* is a common chronical bacterial infection of the lungs in humans. Shortly, it was reported that the atheroma -the fatty, diseased tissue that blocks coronary arteries- often contained *C. pneumoniae*'s DNA and proteins. Coronary heart disease has long been considered an effect of an unwealthy style of life. The usual factors associated to it are well-known: inadequate diet, lack of exercise, tobacco and alcohol. The parallel with *H. pylori* story was obvious and, after the great success in treating peptic ulcer as an infectious disease, the hypothesis that the process of atherosclerosis -the precursor state of CHD- is fueled by a bacterium has an irresistible appeal for some researchers. One of the suggested hypothesis is that a chronic infection may contribute to the risk of CHD by increasing the concentrations of fibrinogin and sialic acid, since these changes favour blood clotting.[3]

Is this a new heresy? Of course. The medical establishment is, again, sceptical. At present, there is no convincing account of the mechanism involved, and very few people are prone to apply a treatment based on antibiotics to heart attack patients. The main contention is that, although we have now compelling evidence of *C. pneumoniae*'s presence in the arteries of patients with heart disease, and not only of its molecular fingerprints, it has not been established that the bacterium causes the disease.[4]

In the end, Warren and Marshall's suspicion proved to be sound so, why do not extend it to other diseases? The traditional factors associated with CHD could not be the actual causes of the disease. Maybe the decisive causal factor is a pathogenic microorganism. In outline, the policy is: look carefully at microorganisms' fingerprints, identify as many of them as you can, and find proofs to blame any of them. Researchers are applying this general policy in other diseases: asthma, schizophrenia, arthritic diseases, sarcoidosis, multiple sclerosis,... The Representational Difference Analysis -one of the techniques employed- compares the *RNA* in diseased and healthy tissues, substracts the sequences common in both, and analyses what is left. Any *RNA* found only in diseased tissue may give some clues to the presence of infectious agents. But this is just the beginning. The hypothesis suggested will not be conclusively established unless we have a fairly good

explanation of what is going on below the surface -what mechanisms and processes are involved in the different phases in the disease-, and the results of the new therapy have been tested. Research is in progress and further investigations may provide telling evidence.

2. The agnostic replacement

According to van Fraassen, an allegiance to empiricism justifies an adamant refusal to go beyond the observable realm. I shall try to sketch his argument. Think about a scientific theory -T- which contains assertions about unobservable entities. While "T is true" entails "T is empirically adequate", the converse is false. T is empirically adequate iff all its observational consequences are true. Then, if T is true, all its observational consequences must be true also. But T may be empirically adequate and, notwithstanding, false. Van Fraassen goes beyond this, of course. He distinguishes between informational virtues and confirmational virtues:

> (...) credibility varies inversely with informativeness. This is most obviously so in the paradigm case in which one theory is an extension of another: clearly the extension has more ways of being false (...) -as I put it then: informational virtues are not confirmational virtues. Indeed the two desiderata cannot be jointly maximized (van Fraassen 1985, p. 280; see also van Fraassen 1983).

For van Fraassen, it is a logical point that by increasing the informativeness of a theory, its chances of being true necessarily get lower. On account of this, whatever be the unobservable entities endorsed, they are a disadvantage concerning truth. *Ex hypothesi*, they evade every empirical test. Besides, since "T is true" has more ways of being false than "T is empirically adequate", belief in T's unobservable apparatus diminishes the likelihood of obtaining truth. Then, we should not believe it: "the assertion of empirical adequacy is a great deal weaker than the assertion of truth, and the restraint to acceptance delivers us from metaphysics" (van Fraassen 1980, p. 69). In contrast to "metaphysical" SR, van Fraassen presents CE as a sound deflationary account which intends to fit the scientific enterprise into the neat pattern of empiricism. CE is a call for reinterpretating science by eliminating the extra baggage which does not afford any empirical gain. The moral is that radical agnosticism -that is, suspension of belief- concerning unobservable entitities is the right epistemic standpoint about science.[5]

Turning now to actual scientific practice, it is not very risky to make a guess about Warren and Marshall's replies if they were asked 'do you believe that *H. pylori* exists?' I think that most scientific episodes involve similar realist commitments. Perhaps scientists endorse a non sophisticated version of realism -something akin to Quine's "unregenerated realism"-, but they are, by and large, realists. Anyway, even if after a survey we were to find that working scientists are realists about the theories they endorse, it would be irrelevant to CE. In principle, Warren and Marshall's beliefs cannot be considered as counter-evidence because CE does not intend to be a *description* of scientists' actual commitments. Indeed van Fraassen warns us not to understand it as a "sociological claim" (van Fraassen 1994, p. 181). So, even though realism is a mental habit deeply rooted in scientists' minds, they should get rid of it because it is no more that an "empty strutting". In fact, van Fraassen's main point concerning scientific practice is that believing in unobservable entities is not necessary to "live" in the theory's world. Using a theoretically-laden language and holding realistic beliefs are different things. Scientists are bound *to speak* a highly theoretical language, but this by no means requires that they *believe* in the existence of the unobservable entities asserted by the theory, because agnosticism does not prevent them from performing all the activities which should be expected from a deep commitment with the theory:

> the working scientist is totally immersed in scientific world-picture (...) But immersion in the theoretical world-picture does not preclude 'bracketing' its ontological implications (van Fraassen 1980, pp. 80-1; see also van Fraassen 1985, section 6).

In sum, accepting a theory does not imply believing it:

> it is possible even after total immersion in the world of science (...) to limit one's epistemic commitment while remaining a functioning member of the scientific community (van Fraassen 1980, p. 83).

Yet we would be misunderstanding CE if we were to think that it involves a radical behavioristic approach to scientific practice. "Scientific practice, (...), is even in just its public aspects intentional activity, and not construable otherwise" (van Fraassen 1984, p. 184). CE does not overlook the doxastic level, for it is intended as an interpretation of scientific practice, and scientific practice is intentional behaviour. Thus, van Fraassen grants that scientists must endorse beliefs about their favourite theories, but he insists that, as far as practice is concerned, all that is needed is believing they are empirically adequate, that is, believing that all their observational consequences are true.

Actual scientific practice very often involves realistic beliefs and CE does not exclude beliefs from scientific practice. Given this, making sense of scientific practice from CE's standpoint implies a sort of belief substitution, namely, *where scientists hold undesirable realist beliefs -that is, beliefs about truth and unobservable entities- replace them by constructive-empiricist tailored ones -beliefs about empirical adequacy and observable entities.* Notice that the plausibility of CE as an account of scientific practice does not depend on scientists' actual beliefs, but on the advantages afforded by this replacement.

How to assess the soundness of such replacement? My opinion is that it cannot be vindicated unless it is shown that:

(i) scientific practice *would* remain the same in case the realist stance on unobservable entities were removed in favour of an agnostic one, and
(ii) the resultant account offers *a deeper insight* into scientific practice than the alternative realist interpretation.

At first sight (i) seems a minimal requirement. The rationale for it is, simply, that what we are trying to understand is the actual scientific practice. If the replacement brought in substantial changes in the standard scientific practice, or if we find any substantial piece of it which cannot be accommodated within CE's framework, we would have a *prima facie* reason to reject CE. Attributing either truth or empirical adequacy to a theory requires, first of all, compatibility with observations. If there is no such compatibility, the theory can possess neither of them. The problem arises, according to van Fraassen, precisely because 'T is true' and 'T is empirically adequate' are empirically equivalent. Similarly, at the metatheoretical level it is compatibility with scientific behaviour what we are concerned with. Belief in truth and belief in empirical adequacy afford rival interpretations of scientific behaviour, but in order to make sense of scientific activity both beliefs must be compatible with the particular practices found in scientific research. In fact, van Fraassen implicitly acknowledges constraint (i) when he insists that agnosticism does not prevent someone from being "a functioning member of the scientific community". For to be an agnostic functioning member means, among other things, to behave in the typical way, that is, in the same way as the non-agnostic members.

If CE fulfills constraint (i) all that can be said is that schewing realistic beliefs has no practical/observational consequences. However this is not enough to affirm the superiority of CE over SR. We can do the agnostic

replacement, but why should we do it? which is the rationale for (ii)? For van Fraassen himself the decisive advantage of CE over SR is that the former yields a better interpretation of scientific practice. Remember van Fraassen's "minor risk" argument. "T is empirically adequate" is logically weaker than "T is true". The former is likelier than the latter, and, given that they are empirically equivalent, the weaker alternative must be preferred. Consequently, scientists who believe only the observable consequences of their favourite theories are better epistemic agents than those who believe they are true -or approximately true-, since they avoid running an unjustified risk.

As I noted earlier, (i) is a basic constraint on interpretations of scientific practice. Certainly, it could hardly be taken seriously any interpretation of scientific practice which supposedly makes better sense of it, but forces us to reject widely accepted research procedures within the scientific community. The next section is precisely devoted to the prospects for CE's replacement concerning the Warren-Marshall episode -which I shall assume is representative of actual scientific practice. I shall argue that agnostic beliefs may be logically compatible with every practice found in it -insofar as they do not *forbid* any of them. However, the whole research programme developed after Warren and Marshall suggested their hypothesis could hardly have taken place if scientists had embraced an agnostic point of view.

3. As-if beliefs

Concerning Warren and Marshall's episode the realist could point out that from the beginning researchers tried to get as much information as possible about the entity discovered by Marshall. Culturing the bacterium, determining its morphology and kinship to other known microorganisms, understanding its internal metabolism, identifying the source of its pathogenicity, and sequencing its genoma, were all crucial stages in this process. The hypothesis that duodenal ulcer is an infectious disease would not have been taken seriously unless researchers had not obtained some success in the initial stages at least. But Warren and Marshall -the realist argues- would hardly have accomplished those initial stages if they had held no realistic assumption about the bacterium.

The realist could still point to a further source of realist commitments. The technique of Representational Difference Analysis, as a general policy to detect pathogenic microorganisms, goes deeper into the unobservable

realm. In effect, scientists seek for traces of RNA left by microorganisms. Could this technique be applied without being committed to the existence of something called RNA? As has been rightly emphasized, scientific techniques and auxiliary instruments are very complex, and as a result, gathering data and testing hypothesis involve an amount of knowledge that exceeds by far a particular scientific field. The conclusion drawn by the realist is that, given the richness in interconnections and the ubiquity of unobservable entities in scientific knowledge, acceptance of any theory -insofar as it assumes a previous test- requires belief in such a kind of entities.

Finally, the choice of a particular treatment makes sense on the assumption that it is going to be more effective than the available alternatives. And this, in its turn, seems to depend on which causes are believed to provoke the disease. That is the reason why doctors give antibiotics to heal duodenal ulcer but do not give them to patients suffering from a coronary disease. Would they have tried such a treatment in spite of suspension of belief about the existence of *H. pylori*? And, what about the effectiveness of the new treatment with antibiotics? After applying it, symptoms disappear and do not recur. Does it not support the claim that *H. pylori* exists and that it causes duodenal ulcer? Apparently, realist commitments are unavoidable.

But now, think about two general practitioners. One of them is a constructive empiricist, whilst the other is a scientific realist. The former believes that '*H. pylori* provokes ulcer disease' is just empirically adequate, namely, that all its observational consequences are true. The realist practitioner, by contrast, believes that there is a bacterium called *H. pylori* in the gut of the patient which provokes ulcer disease. So, the realist practitioner believes that the hypothesis is true -and, *a fortiori*, he believes that all its observational consequences are true, since if any of them were false, the hypothesis would be false too. Notice that the observable symptoms -vomits, pain,...- are entailed by both alternatives. Then, belief in the actual existence of the bacterium is not necessary to infer the observed effects, but it is enough with belief in empirical adequacy. To put the matter in another way, it suffices with believing that the world is *as-if the hypothesis is true* (van Fraassen 1980, p. 21). Accordingly, both practitioners will hold the same expectations concerning observational phenomena because both of them believe the same set of observational consequences. Indeed, believing that the new treatment is effective is no more than believing a subset of O, the set of all the observational consequences of the hy-

pothesis. This subset (O_S) contains observational correlations linking certain observable conditions to the observable non-recurrence of the phenomena called symptoms. Since O_S may be derived from "*h* is empirically adequate" as well as from "*h* is true", the agnostic practitioner will expect that antibiotics are a better choice than H_2 receptor blockers, for instance. In sum, both practitioners *will apply the same treatment* because both of them think that it will be effective.

The decision to apply a treatment is based on expectations concerning what is likelier to occur from the agent's point of view. The efectiveness of the treatment, in contrast, depends on the fulfilment of observational predictions. Thus, in this context expectations are related to what practitioners do and believe while efectiveness depends on which observable phenomena actually occur. Nonetheless, the same line of reasoning could be adduced to explain the effectiveness of the treatment. The realist explanation is, simply, that bacteria have been killed. But there is an as-if alternative: symptoms disappear because all observable phenomena are as-if there is a bacterium called *H. pylori* in the gut which has been eliminated. Again, the elimination of symptoms does not confirm the realistic hypothesis more than its constructive empiricist counterpart.

Warren and Marshall's episode could be entirely rephrased in terms of as-if beliefs. CE's policy in order to cope with it is to replace the alleged necessary belief in unobservable entities by an as-if surrogate with exactly the same set of observational consequences. Further aspects -culturing the bacterium, using RDA technique, even the heuristic policy of firstly detecting the microorganisms' fingerprints and then trying to find out which of them could be related to the disease,...- would be accommodated in the agnostic framework in a way which suggests that, concerning scientific practice, realist beliefs can be dispensed with.

4. Making sense of scientific practice

The foregoing remarks show that as-if beliefs do not function as mere *post-hoc* devices to derive observational consequences. The agnostic scientist's expectations concerning T's future success are the same as those of his realist colleague. Therefore, both proceed in the same way. Believing that the world is as-if T is true seems a powerful option to do the task required by CE. It is general enough to entail exactly the same set of observational (pragmatical) consequences as those entailed by 'T is true'. At first sight, it

supports agnosticism with no loss in empirical content in relation to 'T is true'.[6]

What are the prospects for finding an aspect of scientific practice which cannot be rendered compatible to CE and, at the same time, supports SR? Conjunction of theories, modification of theoretical laws,... have been discussed, but there are no conclusive results.[7] Still, I am sympathetic to this general line of reasoning. In this section, after exploiting all the resources contained in the replacement, I shall try to reinforce the realist's case by pointing to another aspect of scientific practice which I consider an outstanding feature in the Warren-Marshall episode and in scientific research in general. It is my view that CE cannot account for it on pain of throwing out agnosticism.

I begin with a distinction between two different ways of holding that 'The world is as-if T is true'. The fact that the world behaves as if T is true may be understood either as a nomological truth about the world or as a mere coincidence (for this distinction see Kukla 1998, chap. 2). Both interpretations agree that all the observational consequences of T are true, but they differ as to their pragmatic implications. Thus, think about a previously unexamined empirical hypothesis (h_1) which we find it is a consequence of T. According to the first interpretation, if h_1 is an empirical hypothesis derived from T, in principle there is some rationale to believe h_1. At least it is a good reason to investigate it carefully. Now, let us suppose that, after confirming some observational correlations, we notice that they could be deduced from T. According to the second interpretation, the fact that h_1 belongs to the set of empirical consequences of T does not give by itself any rationale for h_1.

Which is the required interpretation of the agnostic replacement to make sense of our example? J.R. Warren used the silver staining method. Something previously overlooked could be detected thanks to this unusual technique in gastric biopsy. Was it an example of serendipity? Had Warren any doubts about the standard medical view on pectic ulcer? I am inclined to think that he did not feel happy with the hypothesis which relates the disease to stress or diet (after all the recidives never were satisfactorily explained). But this is a minor point. Even though Warren's choice concerning the staining method was pure chance, the episode shows that the research took shape after it was attributed a pathogenic effect to the bacterium. CE affirms that science pursues correlations among observable events which increase both accuracy in prediction and control (van Fraassen 1980, p. 31). Plainly, researchers were concerned about such correlations, but empirical

generalizations about the way of contagion, the efectiveness of antibiotics, and so on, were taken seriously insofar as they were related to the unobservable entity supposedly discovered. Those questions were posed only after the theoretical hypothesis -"*H. pylori* exists and it causes peptic ulcer"- was propounded. The subsequent research programme proceeded from this assumption. The fact that an observational regularity were a consequence of the hypothesis became a good reason to investigate it in detail. Thus, in order to cope with the Warren-Marshall episode all that is needed by the constructive empiricist is a nomological interpretation of "The world is as-if T is true" (where T amounts to "*H. pylori* exists and it causes peptic ulcer").

Believing that the world is as-if T is true still seems a good alternative to encapsulate agnosticism and scientific practice. Nonetheless, I think that the nomological interpretation can hardly make sense of scientists' preference for T's observational consequences. Scientists usually pay attention on empirical hypotheses because of the theoretical ones from which they are derived. Besides, testing is a highly selective practice in science due, among other things, to the great amount of human resources and funds invested. Empirical hypotheses may be derived from theoretical ones. Very often they enjoy some initial plausibility which justifies the resources invested. Of course, predictions may fail. But the constructive empiricist is looking for truth, albeit observational truth. It sounds reasonable, then, that he chooses those observational consequences -namely, T's observational consequences- which he himself considers initially plausible. He claims also that suspension of belief in T does not prevent him from deriving exactly the same observational consequences from T. His rationale to focus on T's observational consequences rather than on any other observational generalizations is, again, the fact that the world is as-if T is true. That's all. But if the world is just as-if T is true and we do not confer a reasonable probability to T, why are we going to expect that T's observational consequences will be true? Adopting an agnostic attitude toward T means, among other things, that we believe it could be completely false. Thus, why focusing precisely on those observational consequences? The answer "because the world is as-if T is true" is vacuous, since the world is as-if T is true *iff* T's observational consequences are true. Needless to say that for the scientific realist the plausibility of the observational generalization stems from his previous confidence in the (approximate) truth of the theoretical hypothesis.

Let us take stock. In the previous sections we saw that CE acknowledges that scientific activity is intentional. A doxastic replacement by means of as-if beliefs is urged. But the replacement cannot accommodate the Warren-Marshall episode unless as-if beliefs are interpreted in a nomological sense. The reason is that the agnostic's beliefs about T, whatever they are, should motivate the expectations and behaviour actually found in the episode. Now, we see that the credibility attached to empirical generalizations derived from T, while necessary to explain scientists' decision to investigate them in detail, seems unmotivated from the agnostic standpoint. Certainly, focusing on T's observational consequences is not forbidden by agnosticism. But neither it is encouraged. Considering T true, in contrast, provides a good reason to do it. Such feature of scientific practice puts in danger agnosticism, since we cannot understand similar decisions which are a routine in science.

This all goes to show the limits of agnosticism as an account of scientific activity. At this point, however, we might ask if there is any substantial difference between accepting and believing. According to CE, scientists can legitimally use the theoretical apparatus to derive observational consequences without believing it. They accept T and decide to explore its empirical consequences because of the way the world is (i.e.: because it is as-if T is true). T is just a predictive tool for them. At the same time, T works as a guideline to discern what to look for at the observational level. T somehow anticipates experience and must be taken so seriously as to consider that whatever of its observational consequences deserves a careful investigation. Yet accepting T means not only believing all its observational consequences, but using it to make predictions, design experiments, give explanations, draw fertile analogies in some other scientific fields, deffending the theory against rival accounts and so on. Thus, the causal role of truth-belief is hardly distinguishable from the causal role of empirical adequacy-belief. Both kinds of belief are supported by the same amount of empirical evidence, they generate the same predictions, they play the same role in argument and deliberation,... In fact, formulating theoretical agnosticism to accommodate scientific practice run the risk of turning it into a mere *façon de parler*.[8]

The nomological interpretation of the as-if replacement, although encouraged by scientific practice, does not suffice to account for scientific practice. But if it were, anyway, it would blurr the acceptance/belief distinction because it is dubious that the replacement results in a genuine substitution of beliefs. The constructive empiricist, then, has to face a di-

lemma: the more capable is CE to fit data concerning scientific practice, the more untenable is the acceptance/belief distinction. In order to make sense of the Warren-Marshall episode, realist beliefs concerning T are not dispensable. Now I hope that my initial contention -i.e.: that CE is not an adequate account of scientific practice- has been vindicated.

The agnostic might reply that he does not believe there is a correspondence between theory and reality. Hence, although he accepts T, he does not believe T, as the realist does. But scientific realists are not forced to endorse a metaphysical adequacy of that sort. Believing that T is approximately true or believing that T enjoys some plausibility is enough for them. At least, it is enough for rejecting the radical agnosticism which is at the core of CE.

Realists cannot deny that sometimes scientists make use of theories without believing them. Newtonian mechanics, for example, is a very accurate calculation device when dealing with terrestial physics, and even with many of the phenomena we may find within the solar system. But scientists neither believe newtonian theory is true nor they believe that the world observationally behaves exactly as if it is true. The range of applications for newtonian mechanics is conveniently restricted because physicists do not believe that all its observational consequences are true. Therefore, this situation has little to do with the kind of agnosticism advocated by CE. It is not an acceptance-without-belief counterexample because scientists do not accept -according to van Fraassen's notion of aceptance- newtonian theory, even though sometimes they apply it in specific domains. More compelling examples perhaps could be found in quantum mechanics -not surprisingly, van Fraassen is very fond of it. But even though we grant that the most reasonable attitude in a particular scientific subfield may be suspension of belief beyond the observational level, it would be unfair to extrapolate the features of a particular context to science as a whole. Besides, granting that sometimes an agnostic stance is a wise attitude may be quite easily accommodated by SR, since SR does not claim that we are entitled to believe every unobservable entity postulated by the scientific community. A scientific realist may accept that some of them have been postulated exclusively in order to save the phenomena. It is the constructive empiricist who is in a more difficult position, since he claims that scientists are *never* justified in believing such a kind of entities.

The reader might object that my argument begs the question because it relies on a sort of reasoning -inference to the best explanation- which van Fraassen condemns. Obviously, to endorse the best explanation of scien-

tific practice (presumably, realism), hardly may be effective unless you previously assume the legitimacy of inference to the best explanation. But some remarks are in order here.

To begin with, van Fraassen does not reject inference to the best explanation *tout court*. In *The Scientific Image* his criticism focused on inferences about unobservable entities. In *Laws and Symmetry*, he rejected inference to the best explanation only when understood as a *rule* of inference rather than as a mere inferential *practice*, but he explicitly affirmed that inferring the best explanation may be perfectly valid in everyday contexts (van Fraassen 1989, pp. 149-50). Recently, in a collective paper he restates the views of van Fraassen (1989), but he insisted that what is wrong in inference to the best explanation has nothing to do with observability (Ladyman, Douven, Horsten and van Fraassen 1997). What seems clear is that for van Fraassen inference to the best explanation cannot be outrightly rejected. Rather, what must be discarded are some of the inferences which instantiate the general eschema and/or a particular way of understanding such inferential practices (inferences about the unobservable and/or inference to the best explanation postulated as a "rationally compelling" rule, respectively).[9]

Secondly, it is worth remembering that van Fraassen asserts that CE must be preferred to SR because it "makes better sense of scientific activity". This claim may be understood as an implicit support of explanatory gains, at least in meta-scientific contexts. Certainly, van Fraassen might admit that CE is better than SR as an explanation of scientific activity, but still refusing to believe neither CE nor SR, in line with his view that explanatory virtues are not confirmational virtues. Then, a consistent constructive empiricist should not believe even his own philosophical position. I see no logical incoherence in this, but it sounds really strange.

Thirdly, and finally, I am not deffending that CE is worse, as an explanation of scientific practice, than scientific realism. In a nutshell my contention is that actual scientific behaviour -in particular, scientists' tendency to focus on the observational consequences derived from a theoretical hypothesis- could not be motivated by theoretical agnosticism. Granted that this is a typical feature in scientists' way of proceeding, I consider that the agnostic replacement does not fulfil the basic constraint mentioned in section 2. Therefore, CE is seriously defective as a general account of scientific practice.

5. A new way of doing science?

The Warren-Marshall episode demands scientists which cannot be completely agnostic about T. But so far my argument is concerned only about scientific practice. Our situation may be described as follows. Apart from particular contexts, scientists cannot afford agnosticism. An epistemologist concerned with interpretation of scientific practice has to accept that very often actual scientific behaviour cannot be motivated by agnosticism, so he could not make sense of it without such kind of beliefs. However, an epistemologist interested in the justification of scientific realism could say that, according to my argument, belief in T is just a "psychological lever". After showing that belief in God is necessary to account for religious practice we cannot infer that belief in God is justified. It could be that religious practice stems from unjustified beliefs. Likewise, the scientific realist needs an additional argument to show that scientific practice is not built upon unjustified beliefs about theories. Believing T may be necessary to do some things but T's epistemic justification is a different question. Since we have no more than a pragmatic or psychological vindication of realist beliefs, no progress at all to establish SR has been made.

Admittedly, granting that realist beliefs play an important role in scientific practice is not the same as justifying them. A well-known route for the scientific realist is the so-called "miracle argument": eventual success at the observational or technological level allows us to infer that T is true, because truth is the best -according to any versions, the only- explanation of T's success. This argument has been lengthy discussed for two decades.[10] I am convinced that here antirealism bears the onus of proof. It has to be admitted, however, that antirealists may derive some comfort from the fact that we have no comprehensive account of the relation between the so-called explanatory virtues and the truth. On the other side, a follower of van Fraassen could reply that although current scientific practice were not successfully accounted for by CE, the "minor risk" argument still favours agnosticism. So, even though scientists could not have the motivational strength to behave as they did in the Warren-Marshall episode, realist beliefs should be replaced by as-if beliefs. They would be better epistemic agents anyway.

The "minor risk" argument is worth exploring but is beyond of my scope here, because in this paper I do not intend to analyze all the arguments adduced to support CE.[11] Much less I have tried to offer a full vindication of SR. Rather, I am interested in comparing CE to SR with re-

spect to current scientific practice. That was the challenge raised by van Fraassen himself in the quotation I cited at the outset of this paper, and that was the point of taking an example from recent medical research. Granting that the Warren-Marshall episode is representative of how science is currently done, I think that van Fraassen's claim that CE makes better sense of scientific activity is unacceptable. Theoretical agnosticism does not leave scientific practice as it stands. Indeed, it demands a different way of doing science. Exploring the implications of this way of doing science would require an essay in its own right. Nonetheless, I shall conclude by making some brief remarks concerning this issue.

Agnostic researchers should focus on empirical phenomena. In case they discover that a set of empirical generalizations may be derived from a theory T, they could appeal to T as a way of conjoining what was previously scattered. They could even use the theory to infer further unknown phenomena or empirical generalizations. But all this would not give them any rationale to believe that the observational claims inferred deserve closer attention than any other generalizations which cannot be inferred from T. Moreover, serious questions arise from theoretical incompatibility. Thus, being T_1 and T_2 incompatible theories for a specific domain, the fact that some true empirical generalizations could be derived from T_1 and some other ones from T_2 should not be a matter of concern for agnostic scientists. Leaving aside eventual empirical gains, presumably they should not invest their time in trying to get rid of the incompatibility. Given that theoretical agnosticism is an unsurmountable constraint for CE, a further striking point is that, there is no difference between the situation previously depicted and a different one where the true empirical generalizations of T_1 and T_2 are jointly derived from a unique theory T_3. In any case, we have no more reason to believe T_3 than either T_1 or T_2. We should disbelieve all them.

Could this way of doing science be more successful than the standard way? I would not bet on it. Even though what we are mainly interested in is empirical truth -a goal pursued by theoretical agnostics as well as by realists-, it has to be acknowledged that the standard way of doing science is fairly good at providing more and more accurate empirical knowledge. And, as far as I know, no constructive empiricist denies this.

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Notes

1 See, for instance, the papers of Churchland, Hacking, and Wilson, included in Churchland and Hooker (1985) and Leeds (1996).

2 The residual cases are ulcers caused by other factors, -administration of anti-inflammatory drugs, for instance. For a general survey see Cover and Blaser (1996) and Goodwin *et al.* (1997).

3 See Patel *et al.* (1995). It is worth noting that CHD has been also associated with *H. pylori.* Since the seventies it has been known that duodenal ulcer patients have an increased risk of heart disease and, provided that chronic infection is the main risk factor for CHD, perhaps *H. pylori,* one of the most common chronic infections in humans, is involved in CHD. Recent studies, however, are non conclusive (see Mendall *et al.* 1994 and Glynn 1994). Incidentally, a connection between the bacteria which cause chronic dental infection -the most widespread infectious agents in humans- and CHD has been reported (Mattila *et al.* 1989).

4 Maas *et al.* (1997) reported the actual presence not only of antibodies to *C. pneumoniae* but of the bacterium as well. However, the inexistence of a causal connection is argued in Taylor-Robinson *et al.* (1998).

5 van Fraassen advocates a particular version of bayesianism. Within the bayesian framework he equates agnosticism to vague belief over an interval that includes 0 (van Fraassen 1989, pp. 193-94). Recently, he prefers speaking of "an attitude of negative suspension" toward theories which postulate unobservable entities (van Fraassen 1998, p. 215). See Hayek (1998) and Monton (1998) for some criticisms on van Fraassen's attempt to "bayesianize" agnosticism. Nonetheless, probabilism is not essential for CE. The question at issue is the existence of an epistemic cleavage between observable phenomena and unobservable entities. Van Fraassen's remarks on probabilism just assume this point.

6 Very often 'as-if' locutions allude to the contrast between appearance and reality. For instance, 'It is as if you are at the Moon' -referring to a fair entertainment- implicates that you are not actually at the Moon, although your experience closely resembles to the one you would have if you were there. If there is any reason to think that things are -or could be- different to what they seem to be, an 'as-if' belief is more appropiate than an overt statement of existence. Thus, the locution 'as-if' conveys distrust in appearances and/or tentativeness. The rationale for it could be that the available evidence is poor, that at first sight it clearly points at $\underline{p}$ but a more careful assessment favours $\neg p$, that there is an overt conflict with other well confirmed parts of the doxastic system,

etc. (see Leplin 1987). But the distinctive trait of van Fraassen's agnosticism is that none of these circumstances is a necessary requirement to endorse as-if beliefs and to eschew commitment with unobservable entities. Even in the absence of any especific reason for doubt, agnosticism must be preferred, since it conveys a minor risk. In addition to this, he claims that agnosticism gives a deeper insight into scientific activity.

7 Perhaps realists have underestimated the "agnostic replacement". As an example of an anti-instrumentalist practice, conjunction was initially proposed in Putnam (1975). Van Fraassen's reply is that straightforward logical conjunction does not occur in scientific practice (1980, pp. 83-5; but for a different view see J.D. Trout 1992). P. Railton points out that "decisions about large, expensive, long-lived experimental programs depend for its rationale upon quite strong views about the properties unobservables actually have" (Railton 1989, p. 246). But he does not give any support for this, perhaps because he thinks it is just a platitude. A. Kukla akcnowledges that elimination of theoretical laws could modify the unobservable ontology without altering the empirical content of the theory. However, he asserts that this practice could be justified by pragmatic reasons. Hence it is not a counterexample against the instrumentalistic framework (Kukla 1998, chap. 3).

8 Horwich (1991) makes a similar criticism, but his discussion does not pay attention on as-if beliefs.

9 Recently van Fraassen is not so prone as it was before to support his contention against inference to the best explanation on the observable/unobservable distinction. Concerning observable entities he claims that, although it *seems* that we infer truth, what we really infer is empirical adequacy (there is no empirical way of deciding between both). Then, even in such situations where observable but non observed entities are involved there is no inference to the best explanation. Richmond (1999) cast doubt on this manoeuver. As to the distinction between rule and practice, firstly, few advocates of inference to the best explanation would equate it to inference to the true explanation without further provisos, and secondly, if "rationally compelling" means "logically compelling" they probably would agree with van Fraassen on the inconclusiveness of inference to the best explanation (see also Ben-Menahem 1990).

10 The "miracle argument" was initially suggested in Putnam (1978, chap. 1). Since then it has been extensively discussed. See, for instance, Leplin (1984), the papers of Laudan and Boyd included in Papineau (1996), Cartwright (1983, chap. 4), Devitt (1984, chap. 7), Carrier (1991, 1993), Kukla (1998, chap. 2), Psillos (1999, chap. 4).

11 See Nelson (1996), where it is argued, *contra* van Fraassen, that sometimes informational virtues can be considered as confirmational virtues.

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Valeriano Iranzo is an Associate Professor at the Logic and Philosophy of Science Department of the University of Valencia (Spain). He was Visiting Fellow at The University of Sheffield (U.K.) during 1998-1999. His main research interests are epistemology and philosophy of science and he has written a number of papers on epistemic justification, scientific realism and abductive reasoning.